It may farther be obſerved, that the whole ſurfaces of theſe two ſtones, except the parts of them where they lay in contact with each other, or where they continually preſſed upon the lower ſurface of the urethra, or bed, which they had formed for themſelves in this chanel, are rough, and have ſeveral inequalities, or eminences ariſing from them.

Fig. 2. repreſents the inferior ſmooth parts of the two ſtones, as they appeared when ſeparated from each other, as well as the ſmooth or poliſhed ſurfaces of the ends of theſe ſtones, which lay in contact with each other, and upon motion rubbed againſt each other.

XXXV. *Experiments on the* Tourmalin: *by Mr.* Benjamin Wilſon, *F. R. S. In a letter to Dr.* William Heberden, *F. R. S.*

SIR,

Read Dec. 6. 1759.

I Have the pleaſure to communicate to you ſome experiments made upon the *Tourmalin*, or *Aſhſtone*, which you were ſo kind as to procure me, together with ſome others, reſpecting the vitreous and reſinous electricities, as they are called, and the obſervations I have made thereupon.

The more I am acquainted with electricity, the more I admire a wonderful ſimplicity which ſeems to prevail in nature, at leaſt in this part which abounds with phænomena of a very curious kind; whereof many that have paſſed under my examination of late are ſo extremely nice, that I avoid venturing to relate them, becauſe I would not willingly ſubject myſelf to the cenſure of incautious obſervers.

 I can-

The Royal Society is collaborating with JSTOR to digitize, preserve, and extend access to *Philosophical Transactions (1683-1775).* www.jstor.org

I cannot enter into this particular ſubject, without firſt ſettling a diſpute amongſt electricians, which ſubſiſts at this day, concerning the two electricities; becauſe ſome conſequences, drawn from the ſeveral experiments I have to produce, greatly depend upon it.

Poliſhed glaſs, upon being rubbed properly, has been ſuppoſed to *give* an electricity to bodies, and thoſe bodies that receive it from the glaſs, are ſaid to be electrified *plus*. Whereas wax, amber, &c. upon being rubbed in the ſame manner, have on the contrary been ſuppoſed to *receive* an electricity from bodies, and thoſe bodies which part with it are ſaid to be electrified *minus*. But no experiment, that I know of, has yet appeared to determine which of theſe electricities does really electrify *plus*, and which of them does really electrify *minus*; though it happens that the fact turns out juſt as they have all along ſuppoſed.

In a ſecond treatiſe upon the ſubject of electricity, that I publiſhed in the year 1748, ſeveral experiments were produced, to ſhew that all bodies are ſurrounded with a *medium*, which is *of an exceeding elaſtic nature, and extends* but to *a very ſmall diſtance from the body* when it is not diſturbed by heat, or other cauſes. Since that, other experiments of the like kind have been publiſhed in a work, wherein my late worthy friend *Dr. Hoadly* was concerned with me. Among the proofs therein given, is a curious one, which I obſerved in the *Torricellian vacuum*, where the appearance was remarkably ſenſible. And what is more ſingular, that ſame appearance not only proves the exiſtence of a medium, at or on the ſurface of bodies, but at the ſame time determines which

of

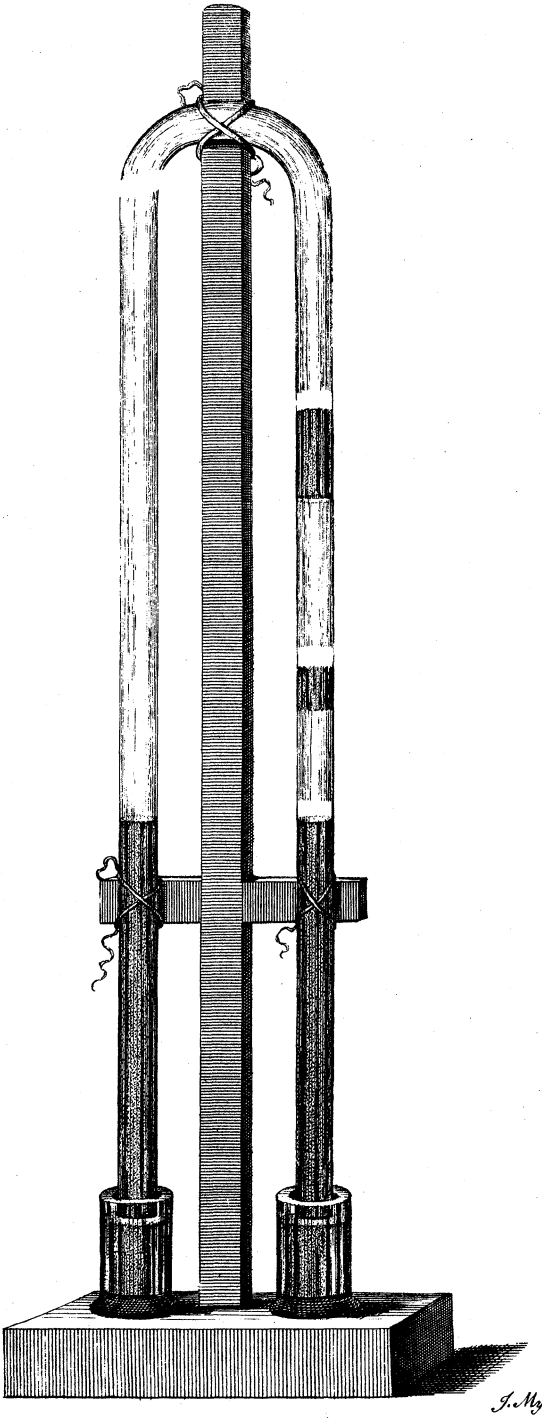
of the electricities is truly *plus*, and which of them is *minus*.

You muſt remember, *Lord Charles Cavendiſh* firſt obſerved the luminous appearance in one continued ſtream throughout the whole *vacuum* of an exhauſted tube. It is a fine experiment, and affords more information than I at firſt imagined, therefore I ſhall beg leave to recite it from the Philoſophical Tranſactions *, before I give you any farther particulars about it.

" This apparatus conſiſted of a cylindrical glaſs " tube of about three tenths of an inch in diameter, " and of ſeven feet and half in length, bent ſome- " what like a parabola, in ſuch a manner, that thirty " inches of each of its extremities were nearly " ſtraight, and parallel to each other, from which " an arch ſprung, which was likewiſe of thirty in- " ches. This tube was carefully filled with mer- " cury; and each of its extremities being put into its " baſon of mercury, ſo much of the mercury ran " out, until, as in common barometrical tubes, it " was in equilibrio with the atmoſphere. Each of " the baſons containing the mercury was of wood, " and was ſupported by a cylindrical glaſs of about " four inches in diameter, and ſix inches in length; " and theſe glaſſes were faſtened to the bottom of a " ſquare wooden frame, ſo contrived, as that to its top " was ſuſpended with ſilk lines the tube filled with " mercury before mentioned; ſo that the whole of " this apparatus without inconvenience might be

* Vol. xlvii.

" moved



J. Mynde sc.

" moved together. The *Torricellian vacuum* then " occupied a ſpace of about thirty inches. In make- " ing the experiment, when the room was darken- " ed, a wire from the prime conductor of the com- " mon electrical machine communicated with one " of the baſons of mercury, and any non-electric " touching the other baſon, while the machine was " in motion, *the electricity pervaded the vacuum in* " *a continued arch of lambent flame, and as far as* " *the eye could follow it, without the leaſt divergen-* " *cy.*"

I imagine *Dr. Watſon*, who has deſcribed this experiment, did not, when he made the experiment, attend to a ſingular appearance of light upon one of the ſurfaces of the quickſilver, becauſe he has taken no notice of it. However, I had a mind to make the experiment myſelf, and try whether I could not manage the quickſilver, ſo as to have more than two viſible ſurfaces, in order that I might have that remarkable appearance repeated.

To do this, I let a very ſmall quantity of air into the tube, by which means four columns of quickſilver were obtained, reckoning great and ſmall together; and fix viſible ſurfaces, three of which I called upper, and three under ſurfaces. But the figure in the plate N°. IX. will give you a better idea of the inſtrument, than I am able to do by writing.

When the column of quickſilver on the left hand was electrified, and the other on the right communicated with the earth, the ſtream of light was viſible in a darkened chamber, and the general appearance, all the way through the *vacuum*, was a light of a ſeeming uniform denſity, excepting at the upper ſurfaces, for above one tenth of an inch, reckoning from

from each ſurface; and there the light was always conſiderably brighter; inſomuch that a gentleman, then preſent, who was unacquainted with experiments of this kind, deſired to know the reaſon of the three *knobs* of light appearing on the tops of the quickſilver. I mention this circumſtance, becauſe they were remarkably conſpicuous; whereas the three under ſurfaces exhibited no ſuch appearance, the light being rather leſs bright than even in the general appearance of the whole illuminated *vacuum*.

An electric current, ſetting in from the glaſs of the electrifying machine, and paſſing along the tube through the quickſilver and *vacuum*, and ſo to the earth, muſt have cauſed thoſe bright *knobs* by means of the reſiſtance the fluid met with at the upper ſurface of the quickſilver in endeavouring to enter it; becauſe the appearances were alike on the three upper ſurfaces, and nothing of the ſame kind was ſeen at the under ſurfaces. Glaſs therefore electrifies bodies *plus*, or, in other words, *gives* bodies a quantity of electric fluid more than they have naturally.

I am now to acquaint you with the appearances that the *minus* electricity occaſioned.

Inſtead of the glaſs cylinder to electrify with, I put a reſinous one, preſerving the communication, and every thing elſe, as in the former experiment. In theſe circumſtances, the general appearance of the light in the *vacuum* was the ſame; but in this experiment the *knobs* of light were on the under ſurfaces, and not upon the upper.

From the knobs of light being at the under ſurfaces, and not upon the upper, I concluded that the flow of the fluid was the contrary way to that cauſed by

by glaſs; for in this caſe it appeared to come from the earth, then into the tube, and ſo on to the reſinous cylinder. Thoſe bodies therefore which have no communication with the earth, and from which the reſinous cylinder is ſupplied, muſt be electrified *minus* *.

Theſe luminous *knobs* (were there no other proofs) are a ſtrong confirmation of the exiſtence of a *medium* at or on the ſurfaces of bodies, which, to a limited degree, reſiſts or hinders the entrance and exit of the electric fluid. But we have ſome other experiments to illuſtrate the ſame, and the *Tourmalin* does aſſiſt us not a little.

There is one thing more, which I am obliged to take notice of, concerning the impermeability of glaſs by electricity. Our friend, *Dr. Franklin*, ſeems to found a great part of his ſyſtem on this opinion.

" Glaſs, ſays he, has within its ſubſtance always " the ſame quantity of electrical fire, and that a very " great quantity in proportion to the maſs. This " quantity, proportioned to the glaſs, it ſtrongly and " obſtinately retains, *and will have neither more* " *nor leſs*, though it will ſuffer a change to be made " in its parts and ſituation; *i. e.* we may take away " part of it from one of its ſides, provided we throw " an equal quantity into the other."

This doctrine I could never agree to, ſo far as it relates to the impermeability of glaſs, as appears by my letters in anſwer to the Abbé *Nollet* and M. *le Roy*, on their requeſting to know my ſentiments about it,

* The ſame cylinder made rough with emmery, and rubbed with flannel at one time, and with leather at another, will afford theſe different appearances, the firſt cauſing the *minus*, and the laſt the *plus* appearance.

in the year 1756, the former of whom it is well known could never come into it*. And I am ſtill the leſs inclined to alter my opinion of this matter, from an acquaintance with the properties of the *Tourmalin*, which I think has furniſhed me with ſufficient grounds for advancing that *Dr. Franklin* is miſtaken. I have wondered that *Æpinus* did not take notice of ſome of the experiments which electrify glaſs either *plus* or *minus*, becauſe the *Tourmalin* affordeth leading experiments towards it; and can aſcribe it to no other cauſe than a favourable opinion he was willing to entertain of *Dr. Franklin*'s hypotheſis.

To prove that glaſs is permeable by electricity, I took a very large pane thereof (I choſe it large, that no objection might be made to the experiment) and then warmed it a little, ſo that it was free from moiſture. After that, I held it upright by one edge, whilſt the oppoſite edge reſted upon wax, and rubbed the middle part of the ſurface with my finger. After this, I found that both ſides were electrified *plus*. Upon repeating the experiment many times, with different glaſs and different degrees of rubbing, ſtill both ſides were in every experiment electrified *plus*. Now they could not both be electrified *plus* by this treatment, unleſs ſome part of the fluid had really paſſed through the glaſs; becauſe the virtue could not get round to the other ſide over ſo great a ſurface: beſides, the extremities of the glaſs were not electrified at all. As to electrifying glaſs *minus*, I find it may be done three different ways. But theſe and other experiments relating to the permeability of glaſs, I ſhall

* Vid. lettres ſur l'electricité par M. l'Abbé Nollet, lett. iv.

forbear

forbear mentioning, till I have brought you better acquainted with the *Tourmalin.*

The firſt account I met with of the *Tourmalin*, and the remarkable properties belonging to it, was from a *memoir in the Berlin acts*, printed in the year 1758; wherein it appears, that *F. V. T. Æpinus*, profeſſor of natural philoſophy, made ſeveral very curious and judicious experiments upon it; the moſt material of which, prove a *plus* electricity on one ſide thereof, and, at the ſame time, a *minus* electricity on the other ſide; provided the *Tourmalin* is moderately warmed, and even by hot water. Theſe appearances are the more extraordinary, as the like means employed in the ſame manner upon diamonds, glaſs, and all other electric bodies hitherto tried, produce no ſuch appearances.

The *Duke de Noya*, who viſited this kingdom in 1758, wrote a ſmall treatiſe on the ſubject, and publiſhed it at Paris on his way to Italy: in this work he mentions *Æpinus*'s experiments, but does not admit of a *plus* and *minus* electricity belonging to the *Tourmalin* when heated. On the contrary, he ſays that the two ſides are electrified *plus*, but one of them is more ſo than the other; and that it is the difference between thoſe degrees which has led *Æpinus* into the miſtake.

I remember to have repeated moſt of the experiments mentioned in the *Berlin memoir*, ſoon after it appeared in England, with the *Tourmalin* belonging to our friend *Dr. Sharp*, which you fortunately recollected to have ſeen in his poſſeſſion many years ago at Cambridge; for it was the only one known of here at that time: and though but a ſmall one, compared

with *Æpinus*'s, yet it was large enough to ſatisfy me that his opinion was well founded. Beſides, the trials you made with the ſame ſtone before I had it, were a farther confirmation of the truth. However, the following experiments, which I made to procure more *data* towards attaining ſome ſimple explanation of theſe curious phænomena, will ſufficiently prove, that one ſide of the ſtone is really electrified *plus*, and the other ſide *minus*. And had the *Duke de Noya* made farther experiments, and purſued the ſame method I have done, I think he would have been of the ſame opinion.

The largeſt *Tourmalin* I had from you, and with which I made the following experiments, weighs above 120 grains. It is of an oval form, and poliſhed: the greateſt diameter meaſures an inch and a quarter, and the leaſt one inch. One ſide is *plain*, the other is *convex*, but cut into ſeveral ſmall planes or *facettes*, ſomething like a roſe diamond; the thickeſt part of which is near one third of an inch. This ſhape does not ſeem to me to be the moſt convenient for making experiments, but I would not alter it, leſt the ſtone ſhould break; for there are ſeveral cracks in it; and I fear it will be difficult to meet with another of the ſame ſize.

To make experiments with the *Tourmalin* requires the greateſt attention, as the appearances ſometimes are ſcarcely ſenſible, inſomuch that I have been obliged to employ the tendereſt kind of apparatus, and even interpoſe a ſort of ſkreen to prevent my breath, or other like motions of the air, from diſturbing the experiment.

My apparatus for making many of theſe experiments, conſiſts of two very ſmall balls made of the pith

pith of elder, and ſuſpended by two linnen threads of the fineſt kind: the ends of theſe threads I faſten to a ſlip of wood about three inches long, and half an inch broad: then upon a ſtick of ſealing wax nine inches long, fixed upright on a table, or any other convenient place, I faſten the ſlip of wood, from one end of which the threads &c. hang down five inches, ſo that the pith balls are about four inches from the table. Theſe balls are always ſuppoſed to be electrified *plus*, except where the contrary is mentioned: but with no greater power, than to make them recede from each other about one inch in every experiment.

I prefer the wax ſtand to a glaſs one, as the latter when cold acquires moiſture very ſoon, and therefore becomes a conductor: whereas wax, when it is once in good order, will continue a non-conductor for a long time.

Before I ſet down my experiments, I muſt mention three truths that are commonly known, and which, for the preſent, I ſhall call

General Laws.

Two bodies equally electrified *plus* recede from each other, or are repelled.

Two bodies electrified *minus* recede alſo from each other, or are repelled.

One body electrified *plus* and another body electrified *minus*, to an equal degree, accede, or move towards each other, or, as it is generally expreſſed, are attracted.

Experiments *upon the* Tourmalin.

Exp. 1. One edge of the *Tourmalin* being properly faſtened to a long ſtick of ſealing wax, I dipped the

ſtone into boiling water, and continued it there near one minute. On taking it out, and preſenting the *convex* ſide thereof near the pith balls, they immediately *receded* from it, but not very ſtrongly.

On turning the *plain* ſide towards the balls, it cauſed them to *accede* to it, but rather at a greater diſtance in this caſe, than they *receded* from it in the laſt. When the *ſtone* was colder, theſe appearances were conſiderably ſtronger; but, on cooling ſtill more, they were leſs and leſs.

Exp. 2. I repeated this laſt experiment, with the difference only of having the balls electrified *minus* inſtead of *plus* *. And in this caſe (as might be expected) the effects were reverſed; the balls *acceding* towards the *convex* ſide, and *receding* from the *plain* ſide.

Theſe appearances in the two experiments agreeing with one another, ſufficiently prove, that *when the ſtone has been heated by water, one ſide is electrified* minus, *and the other ſide is electrified* plus; which is the firſt law laid down by *Æpinus*. This ſtate has not improperly been called its *natural ſtate*; becauſe the *heat*, which diſpoſed the *Tourmalin* to afford theſe appearances, was *uniform* in every part of its ſurface, and the *water* itſelf an *uniform* conductor.

Exp. 3. I preſented the *convex* ſide to the flame of a candle, but not ſo near as to touch the flame, and held it there about one minute, during which time the ſtone acquired a *plus* electricity on both ſides, for

* I did the ſame thing in every experiment where there was occaſion to uſe theſe balls, to be more certain of the concluſions.

the

the balls *receded* from them, but rather with a greater force from the *convex* ſide, than from the *plain* ſide.

This appearance proves an *increaſe of the power in the ſtone*; becauſe it continued to act for a time like other bodies electrified *plus*. And in regard to the different forces of the two ſides, that will be particularly conſidered hereafter.

Exp. 4. After a ſhort time, the *Tourmalin* being colder, and the remaining heat more equally diffuſed, it changed its laſt ſtate to a *plus* and *minus* one; for the *plain* ſide made the balls *accede*, and the *convex* ſide made them *recede*.

This change ſeemed to ariſe from ſome alteration, on or near its ſurfaces, by having the heat equally diffuſed throughout the ſtone: if it was not ſo, I ſee no reaſon but that the *Tourmalin* ſhould continue to be electrified *plus* on both ſides, whilſt any electrical ſigns remained.

Exp. 5. I now held the *plain* ſide as near the flame as I had done the *convex* one; and inſtead of both ſides being electriſied *plus*, they were electrified *minus*, for each ſide cauſed the balls to *accede*.

From this experiment, I had reaſon to believe that the *ſtone* was *emptied* (if I may ſo ſay) of its electricity; becauſe it continued to act for a time like other bodies electrified *minus*.

Exp. 6. After the ſame length of time as in the fourth experiment, the *Tourmalin* being colder, it changed its ſtate alſo; for the *convex* ſide made the balls *recede*, and the *plain* ſide continued to make them *accede*, as they had done before.

This

This change ſeemed likewiſe to ariſe from ſome alteration on the ſurfaces of the *Tourmalin*, by having the heat equally diffuſed throughout the ſtone: if it was not ſo, I ſee no reaſon but that the *Tourmalin* ſhould continue to be electrified *minus* on both ſides, whilſt any electrical ſigns remained.

I was naturally led next to examine at which ſurface of the *Tourmalin* the fluid entered (if any did), whilſt it continued heating.

Exp. 7. *Flame* being improper for my purpoſe, becauſe the electric fluid is readily diſſipated by its preſence, I made choice of an iron rod, at the end of which was a round knob. This was heated, and afterwards brought to a certain diſtance from the balls, in order to ſee if they were moved by it: but not perceiving the leaſt motion, I interpoſed the *Tourmalin*, with the *convex* ſide next the balls. They *acceded* a little, and when I removed the heated iron, they returned to their place again. I then brought the iron nearer to the *Tourmalin* than before: the balls in this caſe *moved* with vigour towards the ſtone, and continued in *contact* with it for a conſiderable time; and, after that, they *receded* from it.

On examining the balls, I found they had loſt all their *plus* electricity, and were electrified *minus*. I alſo obſerved that the *ſtone* itſelf was *minus on both ſides*.

I gathered from this experiment, that the *electric fluid flowed from the balls towards the ſtone*; becauſe they not only loſt their own *plus* electricity, but were electrified *minus*: and, as the *Tourmalin* was *minus* alſo on both ſides, *a quantity of electric fluid muſt have*

have flowed from it towards the iron. That this was really the fact will appear preſently.

Exp. 8. When I heated the *convex* ſide in the ſame manner as I did the *plain* one, the balls were not moved towards the *Tourmalin*, but from it, and continued in that ſtate. In this experiment they were electrified *plus*, and the ſtone alſo was *plus* on both ſides.

I gathered from this experiment that the *plus* electricity in the ſtone did not flow from the balls, becauſe they loſt none of their virtue; but it muſt be *from the iron itſelf.*

It was now neceſſary to examine the iron: but I found it very difficult to do this for many reaſons, and ſuch as might perhaps be thought too tedious if I mentioned them.

I therefore had recourſe to another expedient, where there was a probability of meeting with better ſucceſs. This was to make uſe of a tube of glaſs about two feet long, one end of which I heated red hot and tried the experiments again: obſerving at the ſame time the ſtate of the glaſs after each experiment.

Exp. 9. When the *plain* ſide of the *Tourmalin* was expoſed to the heated end of the glaſs, in like manner as it was to the knob of iron in the ſeventh experiment, I obſerved that about *three inches* of the heated part of the glaſs was *electrified minus*, and beyond that diſtance it was *electrified plus*, and continued ſo even when the glaſs was very near cold.

This *plus* and *minus* ſtate in the glaſs, muſt be cauſed from the action of the fluid flowing from the

balls and *Tourmalin* towards the glaſs: becauſe I found that a current of the electric fluid acting againſt the natural quantity of the fluid in the glaſs will produce the ſame effect: for having rubbed a tube of glaſs, I applied it to the heated end of another tube, and the appearances were exactly alike; that part which was heated being *minus*, and the part beyond it *plus*.

This ſeems a leading ſtep, towards diſcovering the truth we are in ſearch after: for the current of the fluid ſeems to be fairly traced in theſe circumſtances, the heat having diſpoſed the *Tourmalin* properly to let the fluid paſs *from the balls, through its ſubſtance, towards the glaſs.*

Exp. 10. I was now eager to try what would be the event, when the heated part of the glaſs was next the *convex* ſide of the *Tourmalin*. Upon making the experiment, I found that the tube was electrified *minus above one foot in length*, without the leaſt appearance of a *plus* electricity beyond the *minus* one, as in the laſt experiment. And this *minus* appearance continued alſo when the tube was nearly cold.

Now becauſe the *Tourmalin* was *plus*, the balls *plus* alſo, and the heated glaſs *minus*, the electric fluid *muſt have flowed from the glaſs to produce a plus electricity in the ſtone and balls.*

Thus you ſee, Sir, I have diſcovered *two currents of the electric fluid* paſſing in contrary directions, the *one* of them *electrifying both ſides of the Tourmalin plus*, and *the other both ſides minus.*

MY next ſtep was to put the *Tourmalin* into its natural ſtate (as *Æpinus* calls it,) in order to make farther experiments.

Exp. 11.

Exp. 11. To anſwer this purpoſe beſt, I ſeparated the *Tourmalin* from the wax, and placed it in boiling water for a ſhort time, where it was ſurrounded on all ſides with a conductor and an *equal degree of heat:* then, taking it out of the water, I laid the *convex* ſide (after it was dry) upon the ſlip of wood, ſupported by wax, to which the pith balls were ſuſpended; but no appearance happened, for the balls continued at reſt.

Exp. 12. But when the *ſtone* remained on the wood a little time longer, the balls ſeparated to a conſiderable diſtance, ſometimes near two inches; and remained ſo for more than one minute.

In this ſtate they were electrified *plus*, as appeared by their acceding towards amber when it was rubbed and brought near them. From the balls being electrified *plus*, the flow of the fluid from the *plus* ſide of the *Tourmalin* muſt have cauſed ſome part of the wood itſelf to be *minus*, and the balls *plus*; becauſe the ſame effects happened in *Exp.* 9. Beſides, electrified glaſs oppoſed to the ſame wood produced ſimilar appearances.

Exp. 13. If, whilſt the ſtone continued reſting upon the wood, I brought my *finger* near the *plain* ſide of the *Tourmalin*, the balls *receded farther* from each other; and when I repeated the approach, it every time affected the balls and made them recede a little more, unleſs the *Tourmalin* was become too cold; in which caſe they approached nearer, but ſtill continued to be electrified *plus*.

In this experiment, the *finger* did nothing more than ſupply the ſtone with the electric fluid, more readily than the air itſelf would have done.

Exp. 14. Upon removing the ſtone *by degrees* from off the wood, the balls approached nearer and nearer: but when it was taken away entirely they receded again, and in this caſe were electrified *minus* inſtead of *plus*.

This is a farther confirmation that the fluid flowing from the *ſtone* electrified the wood *minus*, by forcing part of the natural quantity in the wood into the balls, and ſo made them *plus*; but becauſe ſome part of the fluid was forced likewiſe out of the balls into the air whilſt they continued *plus*, therefore when the *ſtone* was taken away, they were juſt as much electrified *minus*, as the force of the fluid flowing from the *ſtone* was able to drive out of the balls: and I have formerly ſhewn * that the balls will *recede* from one another in this *minus* ſtate as they do in a *plus* one, by the crouding in of the fluid from the air &c. on all ſides to reſtore the *æquilibrium*; but being retarded in ſome degree in endeavouring to enter the balls by the *medium* on their ſurfaces, an accumulation is formed, and conſequently atmoſpheres ſimilar to the *plus* ones; with this difference only, that one is tending from the body and the other to the body.

Exp. 15. After heating the ſtone again in boiling water, I laid the plain ſide upon the wood. In this caſe the balls continued at reſt as they did in the eleventh experiment, when the *convex* ſide laid upon the wood.

---

* See exp. and obſ. by *Hoadly* and *Wilſon*.

Exp. 16.

Exp. 16. But, after a little time, the balls ſeparated an inch or more, and remained ſo for ſome time. In this ſtate they were electrified *minus*; for they *receded* from the amber.

The *Tourmalin*, in theſe circumſtances, was ſupplied with the electric fluid from the wood and balls, as appears from the preceding experiments, when the heated glaſs was applied; ſo that the balls muſt have been in a *minus* ſtate, and ſome part of the wood in a *plus* one: becauſe of the like reſiſtance at the ſurface to be overcome, where an accumulation of the fluid muſt have been cauſed before it could paſs to the *Tourmalin*; as appears by Lord *Charles Cavendiſh*'s experiment.

Glaſs electrified *minus*, and applied in the ſame manner, produced the like effect. As to the method of electrifying glaſs *minus*, it will be ſhewn preſently.

Exp. 17. Upon bringing my *finger* near the *convex* ſide, the balls *receded farther from each other*, as they did in the thirteenth experiment: and, on repeating the approach, the balls receded a little more, unleſs the *ſtone* was become too cold.

The approach of the *finger*, therefore, conducted the fluid from the *ſtone*, more readily than the air ſurrounding it.

Exp. 18. On removing the *Tourmalin* the leaſt from the wood, the balls approached nearer each other, and continued to do ſo as the *ſtone* was removed farther off; nevertheleſs they were electrified *minus*, though in a leſs degree.

Exp. 19. I then removed it intirely, and the balls *receded*, but to a *greater diſtance than at any time before*

before in the preceding experiments; and, inſtead of being *minus*, they were now *plus*; for the amber cauſed them to *accede*.

By the balls *receding* to a greater diſtance in this laſt caſe than in any other experiment I have yet produced, and the flow of the fluid, during the *natural ſtate* of the ſtone, being from the *minus* to the *plus* ſide, *we have produced another proof, that the reſiſtance is leaſt at the minus ſide of the ſtone:* and from this cauſe, the tendency of the fluid from the balls towards the *ſtone* muſt be greater, than when the fluid tended from the *ſtone* towards the balls. But there is a reſiſtance appertaining to the wood, that was obſerved before, and which muſt be taken into the account, though it is the ſame in each experiment. Nevertheleſs, ſince the tendency of the fluid is different when different ſides of the *ſtone* are expoſed, different degrees of the electric fluid will be accumulated. Hence we ſee the reaſon why, upon removing the *ſtone* from off the wood, as in this laſt experiment, *more* of the accumulated fluid muſt have *flowed* in, than *flowed out*, becauſe the balls were electrified *plus*.

HAVING endeavoured to explain theſe *three different ſtates* of the *Tourmalin*, cauſed by different applications of *heat*, I ſhall now offer ſome farther experiments in which *friction* is concerned, and compare them with other experiments of the vitreous and reſinous kind, in order to obſerve how far they agree with one another, and whether the principles here advanced are conſtant and uniform.

Exp. 20. The *Tourmalin* being again fixed to the wax, I gave the *convex* ſide one ſlight rub with my *finger*,

finger, and obſerved, that both ſides were electrified *plus*.

When the *Tourmalin* was put into its *natural ſtate*, ſo as to electrify *plus* and *minus*, I gave the ſame ſide another ſlight rub; and in this caſe both ſides were electrified *plus*.

Upon repeating theſe two laſt experiments with the *plain* ſide inſtead of the other, the *Tourmalin* was electrified *plus* on both ſides likewiſe; but with this difference, that now they were conſiderably more electrified than before.

This is a farther argument, that *the reſiſtance is leſs on the plain ſide than on the convex ſide*; and that the *fluid paſſed through the ſtone*.

And becauſe ſo *ſlight a friction* occaſioned ſuch a ſenſible alteration, we are ſufficiently cautioned from *touching* the ſtone at any time, but when the experiment requires it. The ſame caution is to be obſerved with glaſs, amber, ſilk, &c.

Theſe experiments put me upon trying the experiment with the pane of glaſs, mentioned in the beginning of this letter; and, upon finding that the *electric fluid* not only *paſſed through the glaſs*, but electrified it *plus*, I had a mind to try, whether I could not electrify it *minus*.

Exp. 21. For this purpoſe I made uſe of the ſame glaſs, and, when it was a little warmed, I held it within two feet of the prime conductor, which was electrified *plus*. By this method, that part of the glaſs, which was oppoſed to the conductor, became electrified *minus on both ſides*; but beyond that, a conſiderable part all round the *minus*, was electrified *plus on both ſides*. This effect is of the ſame kind with

that mentioned in the 9th experiment. In a few minutes, the *minus* electricity diſappeared, and the *plus* continuing, diffuſed itſelf into the place of the other; ſo that now the *whole* was electrified *plus*.

Exp. 22. The experiment ſo far ſucceeding, induced me to make uſe of a leſs piece of glaſs, that I might have the *whole electrified minus*. Upon making the experiment, it anſwered accordingly.

Theſe advances led me to obſerve the power of electrifying this ſmall piece of glaſs and the *Tourmalin* at *different diſtances*.

Exp. 23. I expoſed the ſmall piece of glaſs to the prime conductor, at the diſtance of two feet, which was the ſame as before in the 21ſt experiment, and obſerved a *minus electricity at both ſurfaces*.

Exp. 24. As I moved the glaſs *nearer*, *to a certain diſtance*, it was more ſenſibly electrified *minus*; and after that, on moving it ſtill nearer, the *minus* appearance was leſs and leſs ſenſible, till it came within the diſtance of about *one inch*, and then it was electrified *plus on both ſides*.

Theſe laſt experiments are farther confirmations of the *permeability of glaſs*.

Exp. 25. This *plus* electricity in the glaſs I found might be changed to a *minus* once again, by removing the glaſs, and holding *it for a time at a greater diſtance*: which is another proof of the *repulſive power of this fluid*.

Exp. 26. The *Tourmalin* afforded like appearances when in the ſame circumſtances; with this difference, that they were cauſed at *greater* diſtances than thoſe of the glaſs; and particularly the *plus* electricity was acquired at the diſtance of *one foot*, or more.

From

From this difference in the power at different diſtances, I inferred, that the *Tourmalin reſiſts the exit and entrance of the fluid conſiderably leſs than glaſs, or even amber*; for the diſtances requiſite to cauſe changes in theſe, were leſs, than the diſtances which cauſed changes in the *Tourmalin:* and ſince, by expoſing *glaſs*, and the *Tourmalin*, to an electrified body, at conſiderable diſtances, they are rendered *minus*, and at a nearer diſtance *plus*; it is no inconſiderable argument that their *general laws are the ſame*; and that *the Tourmalin differs in nothing from other electric bodies, but in acquiring an electricity by heat.* And, in regard to this remarkable effect, the experiments I formerly made, which rendered electrics non-electric, as likewiſe Mr. *Delaval*'s curious experiments upon *earthy* ſubſtances, are other inſtances how particular bodies may be ſo altered, as to ſuffer the electric fluid to paſs through them or not, according to the different degrees of *heat* employed in the experiment.

But to proceed with our obſervations on rubbed glaſs.

Exp. 27. Having by me a pane of glaſs, one ſide of which was rough, and the other ſmooth, I rubbed it ſlightly on the rough ſide; upon doing which, *both ſides were electrified minus.*

Exp. 28. I treated the other ſide in the ſame manner; after which, the *minus* electricity was changed to a *plus one, on both ſides.*

Now, becauſe the ſame glaſs afforded different appearances when different ſides thereof were rubbed, and there being no other difference in the circumſtances of the two experiments, except that in the ſurfaces themſelves, the one being rough, and the

other ſmooth; it follows, that the power of electrifying *plus* or *minus ariſes from one and the ſame fluid.*

Exp. 29. I then had a curioſity to try, whether I could not, by rubbing, make one ſide of this glaſs *plus*, and the other *minus*, *at the ſame time.* This I effected, after both ſides were made *plus*; for, by rubbing the rough ſide of the glaſs leſs, than I rubbed the ſmooth ſide, that became *minus*, and the ſmooth ſide continued *plus.* I rubbed the rough ſide *leſs*, becauſe I found from experience, that *rough glaſs* required a leſs power to electrify it *minus*, than *ſmooth glaſs* did to electrify it *plus*; and therefore I concluded, that the *medium on the different ſurfaces* has *different powers, the greateſt belonging to the ſmooth, and the leaſt to the rough ſurfaces*; as Sir *Iſaac Newton* has ſhewn concerning *light* falling upon poliſhed and rough glaſs.

I remember an obſervation of the like kind, which Mr. Short made, upon having occaſion to heat one of his *metal ſpeculums*, behind which was faſtened a wooden handle. This *ſpeculum* he placed near a ſtrong fire, with the poliſhed ſurface towards the ſame, where it continued above an hour, *without receiving the leaſt degree of warmth.* That power therefore, which *reflected the heat*, muſt certainly be of the ſame nature with that, which occaſioned the *knobs* of light in *vacuo*, mentioned in the firſt experiment. I have frequently endeavoured to cauſe alterations in that power, with a view to be better acquainted with its laws; and, amongſt other attempts, I rubbed electrics againſt electrics.

Exp. 30. The firſt trial was with the *Tourmalin* and amber, which produced a *plus* electricity on both

ſides of the *ſtone*, and a *minus* one in the amber. Afterwards I electrified the amber, and held it near the *Tourmalin*; ſtill both ſides were *plus*: and if I rubbed the *Tourmalin* whilſt the amber was electrified, it continued *plus*. Then I rubbed the *ſtone* with glaſs; notwithſtanding which, both ſides of the *Tourmalin* were *plus*, and the glaſs *minus*.

Exp. 31. But when the glaſs was electrified *plus*, and held near the *Tourmalin*, as I had done before with the amber, in this caſe, both ſides were electrified *minus*.

Theſe experiments ſeem to ſhew, that *where electric appearances are produced, by the rubbing of any two poliſhed bodies together*, that body, whoſe ſubſtance is *hardeſt*, and *electric power ſtrongeſt*, *will be always plus*, and the *ſofteſt* and *weakeſt*, always *minus*. It was from this theory, with which I had the pleaſure to acquaint you, that I was deſirous of trying to electrify the *Tourmalin minus* by *rubbing*, not having at that time been able to do it. I fixed upon a *brilliant diamond* for this purpoſe, as being the *hardeſt* body, and *ſtrongeſt electric*, that I was acquainted with; and, upon rubbing the *Tourmalin* with it, my expectations were anſwered; for both ſides of the *Tourmalin* were electrified *minus*, and the diamond *plus*.

Exp. 32. Theſe experiments ſucceeding, I rubbed glaſs againſt glaſs, and found, that *they electrified each other*; but one of them was *plus*, and the other *minus*.

Exp. 33. Two pieces of amber, treated in the ſame manner, were alſo electrified *plus* and *minus*.

Exp. 34. When I rubbed glaſs againſt amber, the former was *plus*, and the latter *minus*.

Exp. 35. Two *Tourmalins* being rubbed againſt each other, one became *plus*, and the other *minus*.

From all which it appears, that ſome alteration was made in the *medium* on their ſurfaces; otherwiſe theſe oppoſite effects could not have been produced: and in regard to the ſame bodies producing different effects, it is not improbable but they may differ in degrees of hardneſs, poliſh, or of their electrifying power.

Now, as *electrics, rubbed againſt electrics, occaſioned electrical appearances*, I was encouraged to try what would be the effect of *air*, if I *rubbed*, or rather *forced*, it againſt electrics; for I ſuppoſed the particles of air to be ſurrounded with a *medium* of the ſame kind as groſſer bodies, which is the cauſe of their being ſo elaſtic.

Exp. 36. To do this, I only made uſe of a common pair of *bellows*, and having brought the *Tourmalin* near to the end of the pipe, I found, after it had *received* about *twenty blaſts*, it *was electrified plus on both ſides*.

Air therefore ſeems to be *leſs electric than* the *Tourmalin*.

Exp. 37. Into the place of the *Tourmalin* I brought a pane of glaſs, and blew againſt it the ſame number of times as in the former experiment. When I examined both ſides, they were electrified *plus* alſo, but leſs than the *Tourmalin*.

Exp. 38. Amber, treated in the ſame manner, was electrified leſs than the glaſs.

Exp. 39. I had recourſe next to a ſmith's *bellows*. The difference theſe occaſioned was only a much ſtronger

ſtronger electricity in the *Tourmalin*. Amber was ſtill weaker than the glaſs, and the glaſs weaker than the *Tourmalin*.

Still having in view the *medium* on the ſurfaces of the particles of air, I conſidered, that *heat* would rarify it; by which means, air, having its reſiſtance leſſened, would more readily part with the electric fluid, and of conſequence electrify more powerfully.

Exp. 40. The *pipe* of the *bellows* being made red-hot, I blew againſt the Tourmalin twelve times only, which was eight blaſts leſs than in the former experiments with cold air. In this experiment likewiſe the *Tourmalin* was electrified *plus* on both ſides, but to a conſiderable degree more, than was done in the 36th and 39th experiments. The hot air had the ſame effect upon glaſs; but electrified it leſs than the *Tourmalin:* and amber, though, like the other bodies, it ſuffered an increaſe of power by the ſame treatment, was electrified the leaſt of all.

From the air electrifying more powerfully when it is hot, than when cold; and the Tourmalin being electrified more than glaſs, and glaſs more than amber, as appears by theſe laſt experiments, we ſeem to have obtained a proof, that the whole atmoſphere is conſtantly promoting a flow of the electric fluid, by the alternate changes of heat and cold: and further, that air is not only leſs electric than the *Tourmalin*, but leſs than glaſs, and even amber.

Exp. 41. When the *Tourmalin* had received the ſame number of blaſts againſt the *plain* ſide, whilſt my *finger* touched the *convex* ſide, it afforded different appearances; for the plain ſide was electrified *plus*, and the other *minus*. After a ſhort time, *both ſides*

ſides were plus; and ſome time after this the *ſtone* recovered its *natural ſtate*, the *plain* ſide being *minus*, and the *convex plus*.

What appeared ſingular in this experiment, was, that the *middle ſtate of the ſtone* ſhould be *plus on both ſides*. But we no more wonder at this, when we conſider, that there were two cauſes to produce theſe effects: the firſt was *heat*, which put the *ſtone* into an *unnatural ſtate* (as *Æpinus* had obſerved before); for, upon cooling, it would recover its *natural ſtate*, and conſequently afford different appearances.

But the ſecond cauſe, which electrified both ſides *plus* when the *ſtone* was in an intermediate (or, as *Æpinus* calls it, *neutral*) ſtate, between the two extremes, was, that then the effects of the air itſelf took place, and electrified *both ſides plus*, as it had done before in the 35th experiment.

Exp. 42. The convex ſide was now preſented to the *bellows* in the ſame manner, and received an equal number of blaſts. In this experiment *both ſides were plus*, but weaker than in the laſt experiment; and, after a time, the *ſtone* returned to its *natural ſtate*, affording a *plus* and *minus* appearance.

This property in the air, of electrifying glaſs, amber, &c. will, in all probability, account for an experiment I met with in *France*, made by *M. Le Monnier* *, who ſhewed me, at *St. Germains en Lays*, two or three long wires, which he had ſuſpended horizontally from the palace to his apartment, above

* See the Memoirs of the Academy of Sciences.

thirty

thirty feet from the ground (the ſame experiment that was made by my friend the *Abbé Mazeas*, and communicated, with ſeveral others, to the Royal Society *), in order to obſerve the *electric effects during thunder ſtorms and cloudy weather*. Theſe *wires* he frequently *found electrified in a ſmall degree, when the day was clear, and without the leaſt appearance of a cloud*. Might not this effect therefore ariſe from *the friction of the air againſt the wires?*

From conſidering all theſe things, and what theſe laſt experiments have taught us concerning the different effects produced by *hot and cold air*, it ſeems probable, that many of the curious operations in nature ariſe from a *conſtant flux and reflux of the electric fluid*. And if the obſervation be true, that air, free from moiſture, during tempeſts and hurricanes in the night, frequently affords a faint kind of light, reſembling what is ſeen in an exhauſted receiver, through which the electric fluid is cauſed to flow, as alſo it occaſions a more general clearneſs in the heavens, than would appear were there no ſuch violent agitations; it is reaſonable to imagine, that *a flux of the electric fluid in the air*, is the cauſe of ſuch appearances; for the ſame reaſon that *our artificial blaſts* produced the electrical effects I have juſt now mentioned.

But it remains to be inquired into, whether the *Tourmalin* is ſo diſpoſed by nature, as to ſuffer the electric fluid to paſs through it only in one direction, like *magnetiſm* through the *loadſtone*, or is indifferent which way it flows.

---

* See the Tranſactions of the Royal Society, Vol. XLVIII.

Exp.

Exp. 43. Upon examining a *Tourmalin* which was *flat on both ſides*, and *poliſhed*, except upon its edges, part of the edge appeared *plus*, and another part oppoſite to it *minus*; ſo that a line, drawn from the *plus* part through the centre of the ſtone to the other ſide, would paſs through the *minus* part.

Exp. 44. Two ſmaller *Tourmalins*, that were flat alſo, and poliſhed, like the other, exhibited the ſame appearances.

Exp. 45. Another *Tourmalin*, which was alſo flat, but *unpoliſhed*, afforded a fourth inſtance of this kind.

Exp. 46. The firſt of theſe *Tourmalins* was afterwards *poliſhed*, as well at the edges as the ſurface; in order to ſee, whether that would make any alteration; but I found it ſtill retained its former electrical ſtate.

Exp. 47. I experienced the ſame with another *Tourmalin*, which had been *rough* likewiſe.

Exp. 48. I then, with a little emery, made that edge, which was *plus*, *rough again*, preſerving all the reſt *ſmooth*; but I could not perceive, that any alteration was made by it. I did the like with the edge of the other poliſhed *Tourmalin*, without being able to obſerve any difference.

Exp. 49. As to the ſmall *Tourmalin*, that is *plain* on one ſide, and a little *convex* on the other, which you obſerved to be a very good one, it is *plus* on the *plain* ſide, and *minus* on the *convex*; which is contrary to the large *Tourmalin* deſcribed in the beginning of this letter.

Exp. 50. I had an opportunity of trying another *Tourmalin*, now in the *Britiſh Muſeum*, which afforded another inſtance of the ſingular diſpoſition of this

this *ſtone*. The *Tourmalin* I now ſpeak of, is *plain* on one ſide, and *concave* on the other; with a ſmall flat border round it. It was ſquare, but one of the corners had been broken off. When I examined it, the broken part was *plus*, and the corner oppoſite to the broken part *minus*: ſo that here alſo the electric current ran through the ſtone in a diagonal line.

Exp. 51. Every one of theſe *Tourmalins*, except that in the *Britiſh Muſeum*, I greaſed all over, and, whilſt they were warm enough to preſerve the greaſe liquid, I tried each *Tourmalin* ſeparately, but found, no alteration in the virtue of the ſtone, except weakening it a little; though it is well known, that moiſture, of any ſort, readily conducts the electric fluid; and therefore, if the *Tourmalin* had not a fixed kind of electricity, the *plus* and *minus* obſerveable on the two ſides of the ſtone, muſt, by this treatment, have united, and deſtroyed each other: the *plus* ſide parting with as much of the fluid, as the *minus*, on the other ſide, wanted, to reſtore the *equlibrium*.

Upon the whole, all theſe experiments do moſt clearly prove, that the *Tourmalin ſuffers the electrical fluid to paſs through it only in one direction, and ſo far it bears ſome analogy to the loadſtone*. And as the *loadſtone loſes its virtue by being made red-hot*, I was deſirous to ſee what would be the event in the *Tourmalin under the ſame treatment*.

Exp. 52. I therefore put one of the flat Tourmalins into a ſtrong fire, for half an hour; but could *not*, afterwards, perceive *the leaſt alteration*. I made the ſame experiment upon another *Tourmalin*, with the ſame ſucceſs.

Exp. 53. Laſtly, I heated the ſtone again, and, whilſt it was red-hot, I threw it into water; by which treatment the *virtue of the Tourmalin was intirely deſtroyed*, and it had the appearance of being ſhivered in many parts, without breaking.

IN regard to the internal frame of the *Tourmalin*, we can ſay nothing; yet ſo much I have learned, by theſe experiments, that there are *three different methods of heating* the *Tourmalin*, which produce different electric appearances; that *different degrees of heat*, afford different appearances; that *friction* has the ſame effect upon it, as upon glaſs; and that the *Tourmalin*, when it is heated properly, ſuffers a current of the electric fluid to paſs through it *in one direction only*: ſo that the *Tourmalin* hath, as it were, *two electrical poles*, which are not eaſy to be deſtroyed or altered; and farther, *that there is not any ſubſtance in nature, which we are acquainted with, that the electric fluid does not readily paſs through*; that there ſeems to be *a conſtant flux and reflux of it in all bodies, as well in the air as in* vacuo, occaſioned by the alternate changes of *heat and cold* in every part of this globe. All which things do very much confirm the opinion I formerly entertained, and attempted to prove by experiment, that the *electric fluid is diffuſed throughout the whole earth*, as well as in *the air* ſurrounding it *.

How far this flowing of the electric fluid may be concerned in the ordinary operations of *nature*, by keeping up that motion, which ſeems ſo neceſſary in the ſeveral parts of the *grand machine*, I leave to others, who may be more ſucceſsful in their reſearches.

* See a Treatiſe on Electricity, the 2d Edit. by B. Wilſon.

By

By theſe advances, we have likewiſe attained to a more certain knowlege of that *medium*, which ſeems to *ſurround the ſurfaces of all bodies*, and which, being of a greater or leſs denſity, or exerciſing (if I may ſo ſay) a greater or leſs reſiſtance, produces different effects, as the *electric fluid* (or *æther*, if you pleaſe) *paſſes in* and *out of bodies*, whenever they are diſturbed by any external violence; and, were it not for this *medium ſurrounding all bodies*, the electric fluid, I apprehend, could *neither be accumulated, nor detained, in any body* whatſoever.

This principle is very ſimple, and ſeems to be of a very general nature. Our great *philoſopher* adopted it from many experiments: he ſuppoſed, that the wonderful phænomena of *nature*, particularly thoſe of *light*, were not to be explained without it; and therefore did not ſcruple to propoſe it, as a principle, to be farther inquired into.

I ſhall think my time well employed, if, by theſe inquiries, I have at all contributed to ſhew, how much we are indebted to this happy *Interpreter* of *Nature*, and have afforded freſh occaſion, by the light of his labours, to admire, and adore, the *firſt cauſe of all things*. But whatever may be the ſucceſs of my endeavours, they have at leaſt been attended with a ſatisfaction, which not a little increaſes that deſire I have ever had for purſuits of this kind. I am,

SIR,

Your moſt obliged humble ſervant,

Great Queen-ſtreet, London,
Nov. 9th, 1759.

Benja. Wilſon.